

March 31, 1947

Mr. Joshua Lederberg
Osborn Botanical Laboratory
Yale University
New Haven, Connecticut

Dear Mr. Lederberg:

Thanks for your kind letter calling my attention to the passage on *Euglena mesnili* in Lwoff's book. I have had the book for sometime and while have not read it thoroughly, the subject of this particular passage was discussed with Lwoff when he visited our laboratory last Summer. Lwoff was quite aware of the resemblance of chloroplast behavior here to the behavior of our cytoplasmic factors, and that is why he called it to our attention. I quite agree. The whole general question will be discussed fully in papers now in preparation in our laboratory, particularly Preer's dissertation which I am to see the first draft of tomorrow. The reference you want is Lwoff, A et H. Dusi (1935a): La suppression experimentale des chloroplastes chez *Euglena mesnili*. C. r. Soc. Biol. 119, 1092.

It seems to me that the similarity between the behavior of our cytoplasmic factors and behavior of plastids and the behavior of intracellular symbionts is inevitable. I have recently thought the matter through as well as I can from the point of view of what the possible moods of behavior of the three classes of objects could be. As a result of this reflection, I am convinced that it is impossible to reach a legitimate conclusion as to whether a particular substance is a symbiont or a normal cellular constituent on the basis of its general behavior in the cell.

Since I saw you last, we have been extending our study of cytoplasmic factors to other characters than the cellular character and have piled up an impressive mass of evidence, confirming my earlier conclusion that this is the normal and regular method of inheritance for all characters in these varieties of *Paramecium aurelia*. It is extremely easy to demonstrate the cytoplasmic factor basis for each character and extremely difficult to find any genic control whatever. Our evidence indicates that our various races of variety 4 are genically alike with respect to most of the characters we are studying and that they differ only in cytoplasmic factors for these characters.

I should be very glad to hear further about the evidence for your single linkage group in coli. This is great stuff.

With best regards and much thanks for your good letter,

Cordially yours,

T. M. Sonneborn

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